ON BACTERIOLOGY

AND ITS RESULTS.

A LECTURE

DELIVERED BY

DR. R. KOCH,

O. Ö. PROFESSOR; GEH.-MED. RATH.; DIRECTOR OF THE HYGIENIC INSTITUTE OF THE UNIVERSITY OF BERLIN, ETC., ETC.,

At the First General Meeting of the Tenth International Medical Congress, Berlin, August 4th, 1890.

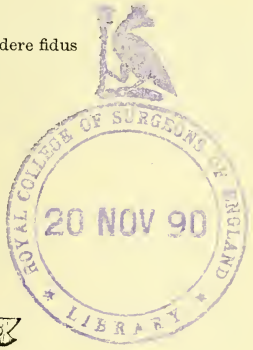
Translated by

THOMAS WHITESIDE HIME, B.A., M.D., ETC.,

Bradford,

AUTHOR OF 'HANDY GUIDE TO THE SANITARY ACTS, FOR MEDICAL OFFICERS, ETC.,' 'CHOLERA: HOW TO PREVENT AND RESIST IT,' 'SCARLET FEVER AND MILK SUPPLY' (PUBLISHED IN THE PARLIAMENTARY REPORTS), ETC.

'Nec verbum verbo curabis reddere fidus
Interpres.'—HORACE.



LONDON:
BAILLIÈRE, TINDALL, AND COX,
20 & 21, KING WILLIAM STREET, STRAND.
[PARIS AND MADRID.]
1890.

ON BACTERIOLOGY AND ITS RESULTS.

When I received the very complimentary invitation to give an address to the International Congress, the question occurred to me whether I should take my subject from that branch of science with which I am chiefly occupied at present, or from bacteriology, to which I formerly devoted myself almost exclusively for some years.

I have determined to select the latter, assuming that bacteriology still arouses the most general interest; and I shall try to give you a short sketch of the present position of that branch of science—at least, in some of its more important features.

I shall not offer anything novel to those who are familiar with the subject; but in order not to appear empty-handed before them, I propose to introduce some facts discovered during the progress of my continued study of tuberculosis, and which have not yet been published.

Bacteriology is a very young science—at least, so far as concerns us medical men. About fifteen years ago there was little more known on the subject than that, in cases of anthrax and relapsing fever, peculiar, strange objects were found in the blood, and that the so-called vibrios occur in cases of infective diseases of wounds. No proof had then been given that these objects were the cause of the respective diseases, and, with the exception of a few investigators, who were looked upon as dreamers, people regarded them rather as curiosities than as possible causes of disease.

Indeed, any other opinion was scarcely possible, because it was

not established that the organisms in question were independent and specifically connected with the diseases. Bacteria had been found in putrid fluids, more particularly in the blood of strangulated animals, which could not be distinguished from the anthrax bacillus. Some investigators even thought they were not living organisms at all, but regarded them as crystalloid bodies. Bacteria, identical with the spirillum of relapsing fever, were alleged to exist in sewage and in the mouths of healthy persons; and micrococci, the same as those which are found in cases of infective diseases of wounds, were said to exist in the healthy blood and tissues.

Indeed, with the means of experimental and optical research which were then at command, it was not possible to advance beyond this point, and matters must have remained long enough in that state had not new methods of investigation been devised, which in a moment entirely altered matters, and opened up new paths into the unexplored regions.

The most minute bacteria were rendered visible by the aid of an improved system of microscopic lenses and proper methods of using them, combined with the assistance of the aniline colours as stains; and by the use of these means the special morphology of each organism could be distinguished.

At the same time, by the employment of nutritive media, liquid or solid as required, it was rendered possible to separate the various organisms from one another, and to obtain pure cultivations, by means of which the specific characteristics of each could be ascertained with certainty.

I was soon able to show what these new methods of investigation could effect. By their aid a number of new, well-characterized pathogenic organisms were discovered, and—a thing of special importance—the causal connection between them and the associated diseases was established.

As all the pathogenic organisms which were discovered were

found to belong to the group of bacteria, it naturally suggested itself that the special causes of infectious diseases are distinct, specific bacteria, belonging to different species; and the hope was aroused that at no distant date the disease-germ corresponding to every infectious disease would be discovered.

This expectation has, however, not been realized, and the further development of bacteriological investigations has in other respects also taken an unexpected development in various directions. I now proceed to consider the positive results of bacteriological research, and would especially refer to the following points.

It must be regarded now as fully established that the bacteria, as well as the higher vegetable forms, represent fixed species, although no doubt sometimes they can only be distinguished from one another with difficulty.

The opinions held only a few years ago with great obstinacy, and, even now, still held by a few investigators, viz., that bacteria are capable of undergoing transformations in a manner totally different from all other living organisms, and can exhibit, now, one kind of morphological or biological characteristics, and, again, others of an entirely different character, and that at most, only a few species must be recoguised; or that bacteria are not distinct and independent organisms, but represent a stage in the development of fungi, or, as some supposed, of lower algæ; further, the theory which still struggles for existence, that bacteria are descendants of animal cells, *e.g.*, of the blood corpuscles — all these opinions are untenable in face of the observations which have now accumulated in overwhelming numbers, and which declare exclusively for the view that in bacteria also we have to do with well-characterized species.

When we consider the fact that some infectious diseases caused by bacteria, such as leprosy and phthisis, have been described in unmistakable language even by the oldest medical writers, we

may conclude that the pathogenic bacteria have a tendency to preserve their qualities during long periods of time, rather than to alter them rapidly, as is commonly assumed, in consequence of the familiar fact that the character of many epidemic diseases varies considerably from time to time.

Doubtless, within certain limits, variations from the usual type of the species may occur in bacteria, and especially in pathogenic bacteria. Still, even in this respect, bacteria do not differ in the least from higher plants, in which many modifications may be met with, commonly referable to external influences, but which induce us at most to speak of them as varieties without our wishing to ignore the species.

Thus it happens that, under unfavourable conditions of nutrition, species of bacteria may produce misformed individuals, whilst other qualities which indeed attract our notice, or interest us from our medical standpoint, but are, perhaps, of little importance for the collective life of the plant (*e.g.*, the production of a colouring-matter; the capacity to grow in the body of a living animal; to evolve certain poisons, etc.) may disappear for a time, or, so far as our experience goes at present, may even become permanently suppressed. But in such cases it is always a question of variations which are included within a certain limit, and never depart so far from the cardinal point of the species-type that a transition into a new or an already known species must be assumed, such, for example, as the alleged transformation of the anthrax bacillus into the hay bacillus.

But since, owing to the minute size of bacteria, we have no well-marked morphological characteristics available for the purposes of methodology at our command, we are all the more compelled, in determining a species, not to depend on any single characteristic with regard to which one cannot beforehand be sure whether it belongs to the fixed or to the variable charac-

teristics of the species in question. On the contrary, we must conscientiously note as many of the characteristics, morphological and biological, of the bacterium under observation, as possible, even though at the time they may appear to be ever so unessential, and we must determine its species according to the portrait completed in this way. In this respect it is impossible to be too careful, and many misunderstandings and contradictions which have arisen in bacteriology have sprung from the, unfortunately, too common disregard of this rule.

The typhoid bacillus offers a very characteristic example of the difficulty to be encountered in determining a species. If it is found in the mesenteric glands, in the spleen or the liver of a person who has died of typhoid fever, no doubt arises that one is dealing with the true typhoid bacillus, because in these organs no other bacteria are ever found which could be mistaken for it.

But it is quite a different matter when the question is one of identifying the typhoid bacillus in the contents of the intestines, in the soil, in water, or in aërial dust. In each of these elements numerous bacilli are found so very like that of typhoid fever that only an expert bacteriologist, and not even he with absolute certainty, can distinguish them from the typhoid bacillus, because undeniable and constant characteristics of this organism are not yet satisfactorily established. The statements frequently made of late as to the typhoid bacillus having been found in the soil, in pipe-water, and in food, can only be regarded with justifiable doubt. The same is true of the bacteria of diphtheria.

A happy chance has, however, ordained that in the case of some other important pathogenic bacteria—*e.g.*, the tubercle bacillus and the cholera bacterium — they possess such unmistakable characteristics that they can be always recognised, even under the most difficult circumstances. The great advantages which

result from the power of diagnosing with certainty the pathogenic organisms of these diseases must act as strong inducements to us, in spite of all fruitless efforts in the past, to strive anew to discover similar unmistakable characteristics for the bacteria of typhoid fever, diphtheria, and other diseases; for by no other means will it be possible to track these pathogenic organisms on their concealed, and often tortuous path outside the body, and to obtain a firm starting-point for rational prophylaxis against them.

But how very careful we must be, even in the case of well-known species, in fixing on those features which are to be regarded as characteristic of a particular bacterium, and as distinguishing it from others, I have myself experienced in the case of the tubercle bacillus. This organism is, as is well known, so distinctly characterized by its behaviour with staining substances, its vegetation in pure cultivations, and its pathogenic properties, and, indeed, is so distinctly indicated by any one of these characteristics, that it would be quite impossible to confound it with other bacteria. And still, even in this case, we should not rely on one only of these characteristics in determining the species, but should follow the established rule, viz., that all the available characteristics must be taken into consideration, and that only when they all correspond is the identity of the bacterium in question to be regarded as established. When I was making my earliest researches as to the tubercle bacillus, I regarded it as most important to follow this rule carefully, and accordingly I tested tubercle bacilli from the most different sources, not only as to their reaction to staining substances, but also as to their mode of vegetation in pure cultivations, and their pathogenic properties. This was omitted only with reference to the tuberculosis of fowls, as at that time it was impossible for me to get fresh material from that source, with which I could make pure cultivations. But as all

other kinds of tubercle had given identical bacilli, and as the bacilli of fowl tuberculosis corresponded with them perfectly in appearance and in their reaction to aniline colours, I thought myself justified in expressing an opinion in favour of their identity, notwithstanding the existence of a flaw in the investigation. I afterwards got pure cultivations from various quarters, which were professedly derived from tubercle bacilli, but varied from the real thing in many particulars; indeed, inoculation experiments made by experienced and thoroughly reliable experimenters with this material which had been sent to me, led to anomalous results, which are even still regarded as unexplained contradictions.

At first I thought it was a question of modifications, such as are not infrequently noticed in pathogenic organisms when they are cultivated continuously outside the body for a long time—that is to say, cultivated under more or less unfavourable conditions. In order to solve the mystery I tried, by means of the most various agencies, to transform the ordinary tubercle bacilli into the supposed variety I have referred to. They were cultivated for months at so high a temperature that a very defective development was induced; in another series of experiments the cultures were repeatedly exposed to a still higher temperature, until they were as nearly as possible killed. I also exposed the cultures to the action of chemical agents, of light, and tried depriving them of moisture; they were cultivated for many generations along with other bacteria, and were inoculated in continuous series on animals having only slight susceptibility to tuberculosis. But notwithstanding all these efforts only trivial modifications were produced in those characters which are constant under similar circumstances in other pathogenic bacteria. It would seem, therefore, as if the tubercle bacillus retained its characteristics with great tenacity. This is quite in agreement with the fact

that pure cultivations of that organism, cultivated continuously in series for more than nine years in test-tubes, and which, consequently, had never during all that time entered the living body, have continued quite unaltered, with the exception of a slight diminution in their virulence.

When every attempt to discover the explanation of the discrepancy had failed, at length an accident cleared up the question. Before the end of the year I happened to get some live fowls suffering from tuberculosis, and I seized the occasion to repeat what I had formerly been unable to do, and I prepared cultivations direct from the diseased organs of these birds. As the cultures grew, I saw to my astonishment that they had the appearance and all the other characters of the mysterious cultures which resembled those of tubercle bacilli. Ultimately I ascertained that these were in fact derived from fowl tuberculosis, and had been regarded as true tubercle, on the assumption that all forms of tuberculosis are identical.

I find a confirmation of this opinion of mine in researches concerning fowl tuberculosis made by Professor Maffucci, which have been recently published.

I do not hesitate to regard the bacilli of fowl tuberculosis as belonging to a distinct species, but one nearly related to true tubercle bacilli. This suggests the very important practical question whether the bacilli of fowl tuberculosis are also pathogenic for man. This question cannot, however, be answered until by continued searching these bacilli have been found in man, or until their absence has been established by a sufficiently long series of cases. But in making such investigations it will not be sufficient to limit one's self, as has been done, to the reaction of the bacilli to colouring agents, but in every case culture-experiments must also be made.

All more recent experience points to the necessity for the most

careful procedure in distinguishing species of bacteria; and for our fixing the limits for individual species, if anything, too narrowly rather than too widely.

In another most important question the conditions have become much plainer and simpler than they formerly were, *i.e.*, in relation to the evidence of the causal connection between pathogenic bacteria and the associated infectious diseases.

The idea that micro-organisms must be the cause of infectious diseases had already been expressed long since by a few leading men, but the majority did not accept the suggestion in a very kindly way; on the contrary, the first discoveries in this direction were regarded by them with scepticism. Hence it was all the more essential to offer irrefutable evidence at the outset that the micro-organisms found in a case of a certain disease are really its cause. At that time the objection was still rightly made that it might be merely a case of the accidental coincidence of the micro-organisms and the disease, and that the former did not act the part of dangerous parasites, but only of harmless ones, which happened to find those conditions necessary for existence in the diseased organs which were not offered to them in the healthy body. Many persons admitted, indeed, the pathogenic properties of the bacteria, but regarded it as possible that they had only been transformed into pathogenic from other harmless micro-organisms, accidentally or regularly present in the body, under the influence of the morbid process.

But if it can be proved—

Firstly, that the parasite is found in every single case of the disease in question, and under conditions corresponding to the pathological changes and the clinical course of the disease;

Secondly, that it occurs in no other disease as an accidental and non-pathogenic parasite;

Thirdly, that when isolated from the body and propagated through a sufficient number of pure cultivations, it can produce the disease anew;

the microbe under these circumstances cannot be an accidental accompaniment of the disease, and no other relationship between the parasite and the disease can be conceived, except that the former is the cause of the latter.

This chain of proof has been completely provided for a number of diseases, such as anthrax, tuberculosis, erysipelas, tetanus, and several diseases of animals—in general, for almost all those diseases which are communicable.

But it has been further demonstrated that in all those cases in which certain bacteria invariably and exclusively occur, they do not behave like accidental parasites, but always act like those bacteria which have been proved to be pathogenic.

We are, therefore, now fully justified in the opinion that although, in any particular case, only the first two conditions of the demonstration may have been fulfilled—*i.e.*, the invariable occurrence of the parasite, and the fact that it is never found in any other disease—the causal connection between the parasite and the disease has nevertheless been established.

Hence we are constrained to recognise as parasitic diseases a number of those with regard to which, up to the present, it has not been possible to realize the third condition of our demonstration, owing to our inability to infect animals experimentally at all, or our being able to do so only in an incomplete way.

Among these diseases are included typhoid fever, diphtheria, leprosy, relapsing fever, and Asiatic cholera. I wish, in this connection, to refer especially to cholera, because the parasitic nature of this disease has been denied with unusual persistency. Every conceivable effort has been made to deprive the cholera bacterium of its specific character, but it has successfully resisted

every effort in this direction, and it may be regarded as a generally recognised and firmly established fact that this bacterium is the cause of cholera.

Besides these general questions, which are of the greatest importance, owing to the general principles they imply, bacteriology has made important advances in various other directions, and has cleared up the question of the relations of the pathogenic bacteria to infectious diseases.

But it would lead us too far if we were to examine these questions in a more detailed manner. It will suffice to observe that we are now for the first time in a position to have correct conceptions of the behaviour of morbific substances outside the body, in the water, the soil, and the air—conceptions which differ considerably from the earlier ones which were held on this subject, which depended on uncertain hypotheses.

We can now for the first time ascertain with certainty how far disease-germs are to be regarded as true parasites—*i.e.*, as parasites which are met with exclusively in the bodies of men or animals; or as parasites which find the conditions for existence outside the body, and only occasionally act as causes of disease. These are points of the very first importance with reference to precautionary measures against certain diseases, especially tuberculosis.

Further, the mode in which disease-germs enter the body has been ascertained for some diseases with sufficient accuracy to enable us to have more correct views on this important subject. With regard also to the behaviour of pathogenic organisms in the interior of the body, our knowledge is becoming constantly more extended, and many pathological processes, which hitherto necessarily seemed mysterious, have been rendered much more intelligible. Among these may be included the common occurrence of combinations of several infectious diseases, of which we

must regard one as the primary disease, and another as the secondary.

The latter imparts to the primary disease an abnormal, unusually severe character, or else it follows it as a *sequela*. This is observed especially in the case of small-pox, scarlet fever, diphtheria, and cholera, also in typhoid fever and tuberculosis.

Further, it is necessary to mention here the results which have been obtained from investigations as to the products of the tissue-metamorphosis of bacteria, as some of them give rise to peculiar toxic effects, and possibly have an influence on the symptoms of infectious diseases, or, indeed, may be the cause of the most important of them. Of especial interest in this connection are the quite recently-discovered, poisonous albuminates—the so-called tox-albumin, which can be obtained from cultures of the bacteria of anthrax, diphtheria, and tetanus.

There is another question, connected with the preceding, which is being investigated with active zeal, viz., that of immunity, a problem the solution of which is only possible with the assistance of bacteriology. No definite conclusion has so far been reached on this subject, but it is becoming more and more clear that the opinion which so long prevailed, according to which purely cellular processes only were involved—a kind of struggle between the invading parasites and the phagocytes which were supposed to act as defenders of the body—is losing ground more and more; and the probability is that in this question, too, the principal part is played by chemical processes.

Bacteriological research has also supplied a large number of facts in reference to the biological processes of bacteria in a relatively short time, and a good deal of the information thus acquired is of importance to the medical aspect of bacteriology. Among these biological facts may be mentioned the discovery of the 'resting' state, as spores (Dauerständen), which is observed in

many bacteria, *e.g.*, the bacilli of anthrax and tetanus. This phase of their existence is distinguished by a power of resistance to high temperatures and chemical agents, which is unparalleled in any other condition of organized living things. The numerous investigations which have been made as to the effects which heat, cold, desiccation, chemical agents, light, etc., exert on sporeless pathogenic bacteria have also given results which are of importance in prophylaxis.

Among these factors light seems to me one of the most important. It has been known for some years that direct sunlight can kill bacteria pretty rapidly, and I can verify this with regard to tubercle bacilli, which are killed in a few minutes or hours, according to the thickness of the layer in which they are exposed to sunlight.

But what appears to me of especial importance is the fact that diffused sunlight has the same effect, though proportionately more slowly: I have found that cultures of tubercle bacilli died in five to seven days when exposed close to the window.

Other important points bearing on the etiology of infectious diseases are, the facts that no bacteria can multiply except in presence of water, or some suitable fluid, and that they cannot pass spontaneously into the air from a moist surface. Hence bacteria can only pass into the air in the form of dust, and only those bacteria which are capable of retaining their vitality for a long time in the dry state can be transported by currents of air, with any chance of causing disease. But they cannot under any circumstances multiply in the air itself, as used to be assumed to be the case with pathogenic substances.

In all the directions so far referred to, bacteriology has fully accomplished all that it seemed to promise at the time of its first development, and, indeed, in some respects it has done more than

was expected of it. But in some other directions it has not fulfilled the expectations which it raised. Thus, notwithstanding the ever-improving methods of staining and the use of lense-systems of constantly increasing angle of aperture, no further knowledge concerning the internal structure of bacteria has been attained than with the original methods.

It is only in the most recent times that new methods of staining appear to have given additional information regarding the structure of bacteria, enabling an internal part, which is probably to be regarded as a nucleus, to be distinguished from an external plasma-membrane, and rendering the motor organs, or flagellæ, which apparently originate in the plasma-layer, visible to an extent previously unattainable.

But in several directions, and just in those where it was to be least expected, bacteriology has left us at fault, especially in the investigation of a number of infectious diseases which would appear to offer themselves as the readiest subjects for our examination, owing to their notable infectiousness. This is particularly true of the whole group of exanthematic infectious diseases, *e.g.*, measles, scarlet fever, small-pox, and typhus fever. For not one of these have we been able to get even the faintest clue to the nature of the specific organism to which they are due. Even vaccinia, which is always at our command and can be so readily tested on the living organism, has obstinately resisted every effort to discover its special pathogenic organism; and the same is true of rabies.

Nor do we know anything as yet of the morbific agents which are the causes of influenza, whooping-cough, trachoma, yellow fever, rinder-pest, pleuro-pneumonia, and many other indubitably infectious diseases. There has been no lack of talent and patience in the employment of every means at our disposal in investigating most of these diseases; and we can only interpret the failure of

the efforts of so many capable investigators as meaning that the methods which have sufficed for the elucidation of so many analogous cases are not suitable in these.

I am, indeed, inclined to the opinion that in the case of the diseases mentioned it is not a question of bacteria at all, but of organized morbific agents, which belong to an entirely different group of micro-organisms. And one feels all the more justified in adopting this opinion since, as is well known, peculiar parasites, belonging to the very lowest group of the animal kingdom, the protozoa, have quite recently been discovered in the blood of various animals, as well as in the blood of men suffering from malaria.

We are not, indeed, quite sure of the evidence regarding these remarkable and most important parasites; and it is improbable that we shall advance much farther until we are able to cultivate these protozoa outside the body in artificial culture-media (as we do bacteria), or learn how to cultivate them under other conditions as natural as possible, so as to be able to study their biological conditions, mode of development, etc.

If this problem should be solved, and there is no reason to doubt that it will be, then it is probable that a collateral branch of bacteriology will be developed, embracing the mode of investigating pathogenic protozoa and allied micro-organisms, and we may hope that it will solve the difficulties surrounding those infectious diseases which have been mentioned, the etiology of which is still unknown.

There is one question which I have so far intentionally left untouched, although it is the one which is most commonly, and not without a certain reproach, addressed to bacteriologists, viz.: Of what practical use have all the labour and pains expended on the study of bacteria been?

Such a question ought never to be asked at all, because true

scientific research pursues its way regardless of whether any direct utility will result from it or not. But I do not regard this question as being entirely unjustified, in the present instance, because only a very small number of those who are occupied with bacteriological investigations have entirely excluded practical results from view.

However, the results hitherto attained by bacteriological research, even if estimated by their practical value alone, are by no means so meagre as the proposers of this question suppose.

I will only remind you of what has been effected with reference to disinfection. Here there was formerly not a single established fact: we acted entirely in the dark, and large sums of money have frequently been thrown away on useless attempts at disinfection, not to mention the indirect damage which is produced by misdirected hygienic efforts.

But now we have in our hands sure indications, by the aid of which we are able to test the efficacy of disinfectants. And although much may still remain to be done in this department, still we may assume that the disinfectants at present in use, in so far as they have proved satisfactory when tested in the laboratory, really do effect their object, when employed in practical disinfection.

We must also include among the practical results of bacteriology its use to control the filtration of water, as there is no other method to employ instead of it. In connection with this must be taken the discoveries made by bacteriological examinations as to the filtering properties of the soil, and the important conclusions to be drawn from them as to the utilization of the Ground-water* for the supply of drinking-water, and as to the proper construction of wells.

Bacteriological methods are also available for the control of

* *Vide* note, p. 23.—T. W. H.

milk, especially in its employment as food for young children, and also for the examination of other kinds of food, and various other articles which are suspected of being infected.

The examination of sewer-air, and the correction of the widespread idea which existed as to its injuriousness, the examination of the air of school-rooms, the evidence of the existence of pathogenic organisms in articles of food, in the soil, etc., are all thoroughly practical subjects, and are all subjects for bacteriological investigation.

I might also add to these practical results of bacteriology the power of diagnosing isolated cases of Asiatic cholera, and the first stage of pulmonary tuberculosis, the former of great importance for the prophylaxis of cholera, the latter for the early treatment of the disease.

But all these advantages are such as are only of indirect value in the war against bacteria. So far we have no direct or therapeutic results of anything like equal importance to place alongside these indirect ones.

The only triumphs which I can mention in this department are those obtained by Pasteur, and others, with the protective inoculations against hydrophobia, anthrax, symptomatic anthrax, and swine erysipelas.

And even with regard to protective inoculations against hydrophobia, which are the only ones so far directly applicable to man, it may be urged that the cause of the disease is still unknown, and that it is probably not even of bacterial nature; and that, consequently, the protective inoculations against this disease should be reckoned to the credit of bacteriology.

However, this discovery, too, undoubtedly grew on bacteriological soil, and could never have been made but for the preceding discoveries of protective inoculations against pathogenic bacteria.

Although bacteriology has such insignificant results to show in this direction, notwithstanding the inexhaustible energy expended on it, still, I do not believe that this will always be the case. On the contrary, I am convinced that bacteriology will yet be of the greatest importance for therapeutics also. For diseases with a short period of incubation and rapid course, it is possible that our therapeutic results will be of less importance. With regard to such diseases—*e.g.*, cholera—the greatest importance will always be attached to prophylaxis. I am thinking rather of other diseases which do not run too rapid a course, because they give more opportunity for therapeutic efforts. And among these diseases there is scarcely one which, partly on this ground and partly owing to its far exceeding all other diseases in importance, so nearly concerns bacteriology as tuberculosis.

It was under the influence of such thoughts that I began to search for agents which might be employed therapeutically against tuberculosis, very soon after I had discovered the bacillus of the disease,* and I have persistently continued my experiments up to the present time, with unavoidable interruptions from official work.

I am by no means alone in the conviction that there must be a means of curing tuberculosis. Billroth has expressed himself in one of his latest publications very distinctly as being of the same opinion, and it is well known that many other investigators are endeavouring to reach the same goal. But it seems to me that many of them do not take the right way in beginning their experiments with man. To this I attribute the fact that every supposed discovery made in this line, from benzoate of soda to hot-air inhalations, has proved illusory.

Experiments should not be begun with the human patient, but

* The discovery of the tubercle bacillus, an ever-memorable event, was announced to the Physical Society of Berlin, March 24th, 1882.—T. W. H.

with the parasite in pure cultures. Even then, when we believe that we have discovered means of checking the development of the tubercle bacillus in cultures in the test-tube, we should not proceed to test the success of our experiment on man. We should first try on animals whether the results obtained in the test-tube are applicable to the living animal, and not until our experiments have been successful with animals should we proceed to apply our method to man.

Acting in accordance with this rule, I have from time to time tested a great number of substances as to their action on tubercle bacilli cultivated in test-tubes, with the result that I have found not a few of them capable of preventing, even in very small doses, the growth of the bacillus in the test-tube. No more than this is required of any therapeutic agent. It is not necessary, as is still often erroneously assumed, to kill the bacteria in the body; it is quite sufficient to prevent their growth and reproduction, in order to render them harmless.

I may mention among substances which are capable of checking the growth of the tubercle bacillus, in very small doses (only to name the most important), a number of ethereal oils; among the aromatic compounds, β. naphthylamin, para-toluidin, xylidin; some of the so-called coal-tar colours, especially fuchsin, gentian violet, methylin blue, chinol yellow, aniline yellow, auramin; among the metals, mercury in vapour, compounds of gold and silver; and the compounds of gold with cyanogen, which are especially noticeable for their efficacy, being capable of preventing the growth of the tubercle bacillus in a dilution of even one to two parts per million.

But all these substances proved perfectly inert when tried on tuberculous animals.

In spite of this failure, I have not allowed myself to be turned aside from the search for means of checking the growth of the

tubercle bacillus, and at last I have found substances which are capable of checking their growth not only in the test-tube, but also in the animal body.

All investigations regarding tuberculosis, as everyone who has tried them knows full well, are very tedious. My experiments with these substances have been so, too; and although I have already been engaged over a year in them they are not yet concluded. I can, indeed, only announce that guinea-pigs, which are notoriously susceptible to tuberculosis, cease to show any reaction to inoculation with tubercular material after they have been subjected to the action of such substances; and that in guinea-pigs which are already in an advanced state of tuberculosis the disease can be brought completely to a standstill without any injurious effects being produced in the animal in other respects.

From these experiments I would, at present, draw no wider conclusion than that the possibility, hitherto rightly regarded as doubtful, of rendering pathogenic bacteria in the living body harmless, without injury to the animal, has been established as a certainty.

But if the hopes founded on these experiments should be realized, and if I should succeed in mastering the microscopic, but hitherto invincible foe within the living body, in the case of one infectious bacterial disease, I have no doubt the same will be effected in the case of other diseases.

Herewith there opens up before our view a most promising field of labour, offering tasks which are worthy of being the object of international rivalry of the noblest kind. Indeed, I have only departed from my habit of saying nothing in reference to incompleted investigations with a view to stimulating others to further exertions in this direction.

And here I would terminate my address, with the wish that the forces of the nations may be arrayed on this field, to do

battle against the smallest but the most murderous enemy of the human race; and that in this struggle for the good of all mankind one nation may surpass another in its successful exploits.

GROUND-WATER, *Note to p.* 18.

(By the Translator.)

As there exists a good deal of misapprehension in this country as to what is meant by Ground-Water, a term long in current use in German, the following remarks may be of service:

'Every kind of soil is found to be more or less damp as we dig downwards, and in most we arrive, as we descend, at a stratum which is evidently saturated with water, because, when some of the clay is removed, water flows into the unoccupied space from the ground around. The stratum which is thus saturated contains no air within the pores, which exist so abundantly in most soils. The water which excludes the air from this stratum is the Ground-Water. The soil above it is, as we have seen, permeable to air and water, and is said to be damp, as distinguished from the saturated stratum. This stratum must, of course, lie on one which is impervious to water, otherwise the Ground-Water would sink. A marsh is a district the Ground-Water of which rises to the surface, and the impervious stratum on which it rests prevents the water draining away. In fact, a marsh may be compared to a gigantic basin sunk in the earth, and filled with peat saturated with water. The basin corresponds to the water-tight stratum. The level of the Ground-Water, instead of rising to the surface as in a marsh, may stand at any height above the impervious stratum, but the nearer it rises to the surface the damper must the superincumbent soil be.' — From 'Cholera: how to prevent and resist it.' Translated from the German of Prof. von Pettenkofer, by T. W. Hime, B.A., M.D., etc. Introduction, p. 19.

Baillière, Tindall & Cox, King William Street, Strand.